describe the apparatus and the methods he employs with sufficient detail; his acquaintance with the psychology of æsthetics is defective; the analogies which he presses between tones and colours are unsound. Yet, despite these manifest shortcomings, the book is to be welcomed as the sincere attempt of an enthusiast, who has spent much time and money on his hobby, to give the world some idea of its interest and of its value. No doubt, seeing is here verily believing; but it is easily imaginable, as Sir Hubert von Herkomer states, that to sit at the author's "instrument and improvise for half an hour whilst watching the ever-varying combinations of colour on the screen produced by the playing is not only an unspeakable delight, but of real health-giving effect on the sense of colour." Apart from his absurd division of spectral colours on the basis of our musical scale, "the main advantages of colour-music as an art" would, as the author rightly says, "remain unaffected, and the force of the chief arguments, which can be advanced in support of it as a separate and distinct art, would not be weakened in the least." It is, as we have said, impossible to describe such æsthetic enjoyment; one must experience it. C. S. M.

Annals of the Royal Botanic Garden, Calcutta. Vol. xii., Part i.:—"Asiatic Palms—Lepidocaryeæ." By Dr. Odoardo Beccari. Part ii.:—"The Species of Dæmonorops." Vol. i., Letterpress. Pp. vii+237. Price Rs. 8 or 12*s*. Vol. ii., Plates. Pp. vii+109 plates. Price Rs. 39 or 2*l*. 18*s*. (Calcutta: Printed at the Bengal Secretariat Press, 1911.)

THE appearance of the first portion of Dr. Beccari's monographic account of the Asiatic Lepidocaryeæ, devoted to the genus Calamus, was recorded in NATURE of August 12, 1909. It affords us pleasure to announce now the appearance of a second instalment of this great work, dealing with the genus Dæmonorops, which, like Calamus itself, consists of "Rotangs," and, among the genera of Palmeæ, is only less important than Calamus because it includes a smaller number of recognisable forms.

The methods and the style adopted in the treatment of the previous genus have been followed in the case of Dæmonorops. These have already been noticed in detail, and, therefore, do not require further discussion. The ample descriptions and excellent illustrations are equal in merit to those in the earlier contribution, and this further instalment of the author's monograph places systematic students once more under a great obligation to him and to the Calcutta Botanic Garden, of the "Annals" of which it forms part. All who are interested in palms will look forward with interest to the conclusion of the task on which Dr. Beccari is engaged, and to which, as the two contributions already at our disposal testify, he has devoted himself with such patient enthusiasm and so great a fund of accurate knowledge.

LETTERS TO THE EDITOR.

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of NATURE. *No notice is taken of anonymous communications.]*

The Principle of Reflection in Spectroscopes.

THE application of a reflector to pass light back through a prism, or prisms, is usually ascribed to Littrow. Thus Kayser writes ("Handbuch der Spectroscopie," Bd. I., p. 513), "Der Erste, der Rückkehr der Strahlen zur Steigerung der Dispersion verwandte, war Littrow" (O. v. Littrow, *Wien Ber.*, 47, ii., pp. 26–32, 1863). But this was certainly not the first use of the method. I learned it myself from Maxwell (Phil. Trans., vol. 150, p. 78, 1860), who says, "The principle of reflecting light, so as to pass twice through the same prism, was employed by me in an instrument for combining colours made in 1856, and a reflecting instrument for observing the spectrum has been constructed by M. Porro."

I have not been able to find the reference to Porro; but it would seem that both Maxwell and Porro antedated Littrow. As to the advantages of the method there can be no doubt. RAYLEIGH.

Acquired Characters and Stimuli.

IN my letter in NATURE of March 21, I pointed out the fact that Dr. Archdall Reid does harm by declaring that the term "acquired characters" as ordinarily used by biologists is not intelligible (is, in fact, nonsensical), giving as his reason that all characters are acquired. That is a "quibble," because the term used by Lamarck (which has been translated as "acquired characters") is "changements acquis," and it is abundantly clear that the *change* spoken of by Lamarck is a change from the normal characters of a wild species. Such normal characters may be, of course, described as "acquired" when considered in comparison with those of the germ from which an individual develops. But that is not the comparison made by Lamarck or by anyone else who uses his term or the English modification of it, and it is a perversion of fact to pretend that it is. It is the plain fact that the *acquired changes* indicated by Lamarck are *changes* as compared with the *normal characters* of the species. There was no allusion in my letter to the terms "innate characters" or "congenital characters." They, of course (as Dr. Reid says), do not mean the same thing as "congenital variation." Dr. Reid in condemning them is beating a mannikin dragged in by himself, diverting attention from the matter in hand. The "acquired changes" or "acquired characters" of Lamarck are properly contrasted with normal characters and not with Dr. Reid's imaginary congenital characters. Considerations as to whether the blacksmith's arm or that of an ordinary man is "normal" are not to the point, since Lamarck was concerned with wild species of plants and animals, of which the "normal specific form" and the "normal specific environment" are understood and known in some detail.

Nor is Dr. Reid justified in attempting to limit the influences under which "acquired changes" or departures from normal specific form are developed to "use and injury." A variety of factors of the en-

© 1912 Nature Publishing Group

vironment, not to be described as use or injury, but broadly classed as excess or defect of heat, light, strain, moisture, chemical constituents of food, may set up in an organism changes of growth, structure, and function of the most striking and obvious character, greatly in excess of the apparent magnitude of the responsible factor. Take, for instance, such cases as that of the rest-harrow grown in dry upland as contrasted with that grown in moist meadowland.

I also objected (and do so again) to the loose use of the word "stimulus" in this connection by Dr. Reid. A particular, definite, measurable agent setting up by its action on living matter a reaction is, in biological terminology, said to stimulate that living matter, and both it and its immediate action are called "a stimulus." The exact nature of the stimulating activity, whether set up by this or that chemical substance, by this or that fluctuation of light, heat, or by electrical conditions, is stated with precision, and its amount and duration compared with the effect on the living matter. To call the nutrition—the normal, persistent nutrition of a growing seed or young plant—"a stimulus" is inadequate and misleading. A good deal of analysis is omitted by so doing. When nutrition, the necessary normal supply of chemical materials in the presence of which a seedling grows and unfolds or develops its specific qualities, is described baldly as "a stimulus," whilst a slicing cut, removing a man's ear and leaving a growth of scar tissue in its place, is also dismissed as "a stimulus," it is obvious that two things profoundly different in character and importance are confused under a common heading. The first is the absolutely essential and widely distributed condition for the continued existence of a living thing; the second is exceptional—an abrupt change with correspondingly exceptional result. Neither is correctly described as "a stimulus," though many stimuli of different nature occur in connection with both.

Dr. Reid says he will admit that he is quibbling about the meaning of the term "acquired characters" if I will indicate how an inborn trait is more inborn and less acquired than an acquirement. The term "inborn trait" has nothing to do with the matter, as I have explained above. The words "change" and "acquire" imply an existing standard *from* which there is change or *to* which there is addition. The fact that the standard is itself an acquirement when viewed in relation to another phenomenon, namely, a reproductive germ, is irrelevant.

Dr. Reid quotes passages from Wallace, Weismann, and Romanes which do not treat of the matter under discussion, and suggests that he "sins with them," and that they agree with his forced interpretation of the term "acquired characters." The suggestion seems to me to be devoid of justification.

Chiefly, however, I object to Dr. Reid's stating that I have called this "a historical discussion," implying that I attach historical importance to it. I have used no such words. This statement by Dr. Reid is erroneous, as is also his attribution to me of certain opinions about the muscular development of an ordinary individual and of a blacksmith. He says, "Sir Ray Lankester regards the former as normal and *therefore* inborn and inheritable, and the latter as abnormal and *therefore* acquired and non-inheritable." This is entirely imaginary. I never wrote a word on the subject of muscular development, nor have I stated that abnormal qualities are necessarily acquired and non-inheritable, or anything of the kind. I do not desire to continue a discussion in which fictitious words and opinions are attributed to me. Nor do I desire to obtain any "admission" from Dr. Reid. I am content to leave the matter to the judgment of your readers.

April 5. E. Ray Lankester.

Clouds and Shadows.

On the evening of Easter Monday I noticed in the western sky an effect which was unlike anything I had ever seen before. The sun was just setting behind a great bank of cloud, the rest of the sky being fairly clear, except for a thin veil of alto-stratus (it was not very high), which was moving at a good rate from the north-west, and stretched across the whole sky. This stratus was scarcely noticeable at first, as the sun's rays shining through it produced a milky kind of light in the sky. In startling contrast to this there appeared about halfway between the horizon and the zenith, to the south-west, what looked like an extraordinary "cloud," which compelled attention. It was obvious, however, that this was no cloud, as it remained quite stationary, while the stratus (which I now observed) and also a few small lower clouds were driven quickly across the sky.

I became greatly interested in the phenomenon, and watched it closely for half an hour or more, and the impression I got was that the apparent cloud was really a heavy shadow, cast upon the otherwise brightly illumined stratus by some unseen object away in the west, which was intercepting the sun's rays. The "dark patch" varied in shape and size, expanding and contracting, but preserving on the whole a shape somewhat like a fan, and keeping the same position in the sky.

After a time I noticed an exactly similar effect growing into shape, halfway between the first one and the point where the sun had set, so that a line drawn through them from the sun would be at an angle of about 45° with the horizon.

I made notes and rough sketches at the time, and could give more complete details as to the conditions existing, and the varying shapes and positions of the dark "shadows." It may be that this effect is not uncommon, and is easily accounted for; but although I have studied the skies for many years I have never before seen anything like it, and I feel confident that it must have been, at any rate, unusual.

While freely confessing ignorance of any scientific knowledge on such matters, I should be very glad to be enlightened as to the explanation of the phenomenon, and also to hear whether anyone else noticed the occurrence.

Chas. Tilden Smith.

"Chisbury," Little Bedwyn, Wilts, April 15.

Winter in India.

I notice that in Nature of February 15 your reviewer quotes without comment a passage from "Freshwater Sponges, Hydroids and Polyzoa" (Fauna of British India Series) which implies that winter in India is the *driest time* of the year as well as the coolest. This must be a slip on the part of the author. Not only is there a considerable quantity of water in rivers, tanks, and pools in winter compared with the spring and early summer, but the relative humidity is very much higher. In cases where I have collected figures the mean relative humidity is at about the average of the whole year in December and January, and then drops continually up to the first half of May, but it would doubtless vary in different parts of the country. H. H. H.

Camp, Central Provinces.

© 1912 Nature Publishing Group